M. Coulier made experiments with the products of combustion from flames in which the combustion was as perfect as possible. He found these gases much more active than the air of the room. This he attributed to particles of unconsumed carbon. He also found the air after rain and storms to be less active, and the air in summer less active than in winter. After extending the experiments to alcohol and benzine, the paper concludes with some remarks on the peculiar action of ozone.

Up to this point the two investigations run perfectly parallel, and the strange likeness between the two sets of experiments is not the least interesting point connected with them. After going over this first paper by M. Coulier, I found he had communicated a second paper, which will also be found in the same volume of the *Journal de Pharmacie et de Chimie*, at page 254. This second paper is almost entirely occupied with a description of some experiments in which inactive air was heated and rendered active.

In the first experiment described in M. Coulier's second paper a platinum wire was heated in the purified air of the flask, after which the air was active. In the second experiment pure air in which hydrogen was burned became active. In the third experiment pure air which was passed through a glass tube surrounded with tinsel ("clinquant"), and moderately heated, was made active. Fourth experiment, oxygen, nitrogen, and hydrogen became active after they had been heated in a tube. After describing some effects in ventilation when *highly* heated air is used, he says, "In the preceding note (the first paper) I believed I could attribute the activity of the air to the presence of solid bodies, and it seemed to me that the only solid body that could escape from a carbon flame could be nothing but carbon itself. It was the remarkable experiment, so easily made, of filtering air through cotton-wool, that led me to form this hypothesis, which the experiments above related invalidate (à faire cette hypothèse, que les expériences relatées plus haut infirment)." He concludes by saying, "The explanation of these phenomena remains still to be found."

Experiments exactly corresponding to some of those described in M. Coulier's second paper will be found in mine. Wishing to test the effect of combustion on air, I first made experiments to test the effect of heat on the apparatus to be used in collecting the hot gases. For this purpose I passed filtered air through a heated glass tube, after which I found it was remarkably active. It was found however that this activity is not due, as M. Coulier seemed to suppose, to the heating of the air, but to impurities driven off the surface of the tube by the heat. This was proved by showing that the air remained inactive when the hot tube through which it was passed was thoroughly cleansed.

In making experiments on the effect of burning gas I arranged a platinum wire, connected with a battery, to enable me to light the gas in the pure air of the receiver. On testing the action of the heated wire alone, it was found that simply heating the wire gave rise to cloudiness. It was however found that by highly heating the wire its activity was destroyed, all impurities being driven off.

These experiments explain M. Coulier's first and third experiments. The fourth experiment is also to be explained by the nuclei driven off the tube by the heat. These nuclei may be driven off in the solid state, or as gases which condense without nuclei when *highly* supersaturated on being cooled to the temperature of the flask. The nuclei are in some cases formed by chemical union of the gases driven off by the heat, and in other ways unnecessary to enter upon here. As to the second experiment, more information is required as to arrangement of apparatus, &c., before any opinion can be formed as to the origin of the nuclei.

It now appears to me that this second paper explains why the first results of M. Coulier, though repeated and confirmed by M. Mascart, have not received that general acceptance we should have expected. In his second paper he describes a number of results which he did not succeed in fitting into his hypothesis. They even seemed to him to shake his first conclusions, and the uncertain sound given by his second paper seems to have blighted any fruit his first paper was likely to have produced. There can however be no doubt that M. Coulier was the first to show the important part played by dust in the cloudy condensation of the vapour in air, and his first paper clearly explains its action. It seems highly probable that if it had not been followed by his second paper, or if he had succeeded in getting the key to the explanation of his experiments, and his conclusions had confirmed instead of weakening the teaching of his first paper, his result would long ere now have been applied to explain the different causes and the different forms of cloudy condensation in our atmosphere, as well as other physical phenomena.

Darroch, Falkirk, February 15 JOHN AITKEN

Geological Climates

I DESIRE to express my thanks to Dr. John Rae for the valuable contribution of "facts" which he has added to this interesting question, of which I hope to make use in due time.

I wish also to answer the question asked by Prof. Woeikoff in his letter of February 17. My authority for January, July, and mean temperatures in the northern hemisphere and in the southern is the most recent and accurate available, viz., United States Coast Survey, "Meteorological Researches for the use of the Coast Pilot," Part 1, by William Ferrel (Washington, 1877). Mr. Ferrel gives the January and July temperatures for every ten degrees of longitude and latitude, up to 80° N. and 60° S. as follows, so far as regards the annual means :—

Lat. N.	Annual.	Lat. S.	Annual.
0°	80°·1 F.	0°	80°·1 F.
10	81·0 ,,	10	78·7 ,,
20	77·6 ,,	20	74·7 ,,
30	67·6 ,,	30	66·7 ,,
40	56·5 ,,	40	57·9 ,,
50	43·4 ,,	50	47·8 ,,
60	29·3 ,,	60	35·3 ,,
70	14·4 ,,	70	—
80	4·5 ,,	80	—

This table fully justifies what I said of the southern hemisphere as compared with the northern, and is, of course, explained by the existence of three great gulf streams in the south, which raise the mean temperature, producing insular climates with a small range from July to January.

Mr. Ferrel adds, at the close of his discussion (p. 22) :—

"From Dove's Charts of Isothermal Lines, which do not extend beyond the middle latitudes in the southern hemisphere, it has been inferred that the southern hemisphere is colder than the northern, and this has been the accepted view ever since his charts were first published, in the year 1852; but from the results obtained above it is seen that the mean temperature of the southern hemisphere is the greater of the two."

I was well aware that the east coast of Asia is colder, latitude for latitude, than the east coast of North America, but this has nothing to do with reducing the temperatures of the east coast of America, by means of alterations in the ocean currents of the North Atlantic, which I deny to be possible.

SAMUEL HAUGHTON

Trinity College, Dublin, February 19

Climate of Vancouver Island

As questions connected with the climate of Vancouver Island and the influence on it of ocean currents have lately been the subject of several communications in the pages of NATURE, it may be worth while to draw attention to the fact that Esquimalt, at the southern extremity of the island, together with several places on the mainland of British Columbia, have now been for a number of years occupied as regular stations of the Canadian Meteorological Service, and that trustworthy meteorological results are to be found in the annual reports to Government.

When writing a report on British Columbia for the Canadian Pacific Railway Survey in 1877, I applied to Prof. Kingston, then in charge of the Meteorological Department, for some information on climate, and received from him an abstract, which was published at the time ("Can. Pacific Ry. Report, 1877," p. 246), by which it appears that the mean summer temperature of Esquimalt is 57°·82 F., mean winter temperature 34°·45, mean annual temperature 47°·97. This does not include however the additional results of the last few years.

Much information on the climate of the northern part of the north-west coast may also be found in the *Alaska Coast Pilot*, 1869, and the *U.S. Pacific Coast Pilot*, Appendix 1, 1879. In the latter, series of monthly and mean annual isothermal lines are given for the air and sea surface, which—though the observations at command are by no means complete—are doubtless nearly correct. A partial abstract of these, with some discussion

of the climatic features of British Columbia, may be found in an appendix written by me for the Canadian Pacific Railway Report of 1880, p. 107.

The mean temperature of Tongass at the southern extremity of Alaska, from two years' observations, is stated as 46°·5.

Observations have been maintained at Sitka with little interruption for a period of forty-five years. The latitude of this place is 57° 3′, or about one degree north of Glasgow. The mean temperatures are as follows:—spring 41°·2, summer 54°·6, autumn 44°·9, winter 32°·5, and for the year 43°·3.

According to the *Pacific Pilot* above quoted, that portion of the Kuro-siwo, having a temperature of 55° F. or more, approaches the coast in the vicinity of Vancouver Island. Temperatures not much lower than this however prevail much further north. The average temperature of the surface of the sea during the summer months in the vicinity of the Queen Charlotte Islands as determined by me in 1878 ("Report of Progress, Geological Survey of Canada, 1878-79") is 53°·8. Observations by the U.S. Coast Survey in 1867, in the latter part of July and early in August between Victoria and Sitka, gave a mean surface-temperature of 52°·1.

GEORGE M. DAWSON

Geological Survey of Canada, February 1

"The New Cure for Smoke"

IT was not my intention to trouble you further on this subject at present, but as Dr. Siemens has been good enough to notice the result of my trials with the coke-gas grate, and has asked a question with reference to the grate used by me, it is due to that gentleman that I should at once explain that the grate in which the trials were made is of modern construction and permanently fitted with side-cheeks and back of fire-clay lumps, and that when in use with the coke and gas the back was fitted with a copper plate, and in all other respects the grate was arranged in the manner described and illustrated in NATURE, vol. xxiii. p. 26.

J. A. C. HAY

On the Space Protected by Lightning-Conductors

THE very interesting article by Mr. W. H. Preece on the "Space Protected by a Lightning-Conductor" (*Phil. Mag.* 5th series, vol. x. p. 427 *et seq.*, December, 1880) revives this important practical question. The old rule, first enunciated by M. Charles, which makes the radius of the protected circular area around the base of the rod equal to twice its vertical height, has never been satisfactorily verified either on theoretical or experimental grounds. This rule was adopted in the Report of the Commission of the French Academy of Sciences drawn up by M. Gay-Lussac in 1823 (*Ann. de Chim. et de Phys.* 2nd series, t. 26, p. 258), and also in two other reports drawn up by M. Pouillet, one in 1854 (*Comptes rendus*, t. 39, p. 1142), and the other in 1867 (*Comptes rendus*, t. 64, p. 102). But still more recently the Committee appointed by the Préfet de la Seine to superintend the construction of lightning-conductors in the City of Paris, in their Report in February, 1876, *reduced* the radius of the protected area to 1·45 times the height of the rod. I am ignorant on what grounds the Commission adopted this precise number.

In this state of the problem Mr. Preece's paper was both apposite and welcome. The rule which he deduces certainly has the merit of *definiteness*; but it seems to me that it fails to be *practically satisfactory*. For it is very evident that his investigation is exclusively applicable to "*Blunt-Conductors*," since the "*Power of Points*" is entirely left out of consideration. His deductions might apply to the *blunt*-conductors which crowned the Royal Palace of George III., but are scarcely applicable to the *pointed* rods now employed! His investigation assumes that the *distance* of the earth-connected objects from the electrified cloud is the *only element* which determines the *direction* of the discharge. It seems to me that the well-established "power of points" to discharge, or rather to *neutralise* the electricity of charged conductors, is an essential element in the problem of the protected space.

It is a well-known fact that when an electrified cloud approaches a *pointed* lightning-conductor which is in good conducting connection with the earth, the sharp point becomes charged by induction with opposite electricity of *high tension* long before the distance between them approximates that required for a *disruptive* discharge; so that electricity of the opposite kind from that of the cloud *escapes* from the point in the form of a *connective discharge* or electrical *glow*, and *neutralises* that of the cloud, and thus silently *disarming* it, *averts* the disruptive stroke of lightning. This *neutralisation*, due to the power of points, constituting the *preventive action* of lightning-conductors, is justly regarded as the *most important* function of such rods; although, under certain extraordinary circumstances, they may be forced to carry disruptive discharges. Under any circumstances, however, it is obvious that *pointed* conductors must *enlarge* the protected area as compared with *blunt* conductors.

It is very difficult, if not impossible, to estimate in a precise manner how this *power of points* would *modify* and *distort* the *equipotential surfaces* in the intervening electric field. The problem is evidently one of great complexity. The following circumstances must obviously influence, to a greater or less extent, the *magnitude* and *direction* of the resultant electromotive force, which determines the path of discharge, *convective* or *disruptive*, viz.: (1) *Distance* of thunder-cloud from the point of the conductor; (2) variable *dielectric properties* of the intervening air; (3) *size* of the cloud; (4) the variable *tension* of its electric charge, especially under the neutralising action of the pointed rod; and (5) the *velocity* with which the thunder-cloud approaches the point of the conductor. The *last* consideration is very important, and at the same time most difficult to formulate; for the *convective neutralisation* is a *gradual* process requiring *time*. It is evident that a heavily-charged thunder-cloud *rapidly* driven towards the point of the conductor might give rise to a *disruptive* spark, while, if *slowly* approaching the same, it would have been *silently neutralised*, and the stroke averted. In fact the strength and direction of the resultant force is influenced by so many variable conditions that it would tax the resources of a powerful calculus to indicate a formula which would satisfy, even approximately, the demands of practice in the construction of lightning-conductors.

Nevertheless, it is quite certain that Mr. Preece's rule, which makes the radius of the protected circular area equal to the *height* of the rod for *blunt* conductors, is perfectly *safe* for *pointed* rods; for there can be no question as to the fact that the "power of points" *enlarges* the protected area.

The late Prof. Henry frequently witnessed the efficacy of convective discharges from the point of the lightning conductor attached to the high tower of the Smithsonian Institution. During violent thunder-storms at night, at every flash of lightning he observed that "a jet of light, at least five or six feet in length, issued from the point of the rod with a hissing noise."

It is proper to add that while the circumstances influencing *disruptive* discharges of electricity have been experimentally investigated by a number of physicists, the laws of *convective* discharges from points do not seem to have received attention from any experimenter. Thus I have not been able to find a satisfactory answer to the following elementary inquiry, viz.—Under given conditions, at what *distance* will a pointed conductor connected with the earth *begin* to neutralise the electricity of an insulated conductor by the convective discharge of the opposite kind of electricity from the point?

In short, the whole subject of the "power of points," although one of the best-established and most conspicuous phenomena in electricity, is sadly in need of experimental investigation. This class of electrical phenomena is pretty much in the same condition in which Franklin left it more than a century ago.

Berkeley, California, January 1 JOHN LE CONTE

[Mr. Preece has shown by considering the area between the conductor and the charged cloud as an electric field mapped out in equipotential surfaces and lines of force, that "a lightning-rod protects a conic space whose height is the length of the rod, whose base is a circle having its radius equal to the height of the rod, and whose side is the quadrant of a circle whose radius is equal to the height of the rod."—*Phil. Mag.*, December, 1880.—ED.]

Localisation of Sound

MY friend the Rev. H. J. Marston, Second Master of the School for Blind Sons of Gentlemen at Worcester, has communicated to me some very singular instances of the power of localising sound possessed by blind boys.

One of the games in which his pupils most delight is that of *bowls*. A bell is rung over the nine-pins just as the player is ready to throw the bowl, when, totally blind as he is, he delivers it with considerable accuracy of aim. Mr. Marston vouches for the fact that it is no uncommon feat for a boy to strike down a single pin at a distance of forty feet three times in succession.